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ABSTRACT

This booklet is based on a series of 1971 conferences attended by 22 prominent individuals in the field of child development research. Conference participants met in three working panels (on infancy, the preschool child, and the school age child) to assess the current status of the child development research field and to project research needs for the coming decade. Representing the collective opinions of the panelists, the booklet describes trends in research, gaps in research activity, and barriers to research efforts. The priorities in child development research are discussed, and the consensus on research needs is reported under the following headings: (1) the need for more and better communication in research and development, (2) the need for collaborative research, and (3) the need for certain types of manpower. In addition, the synthesis of research results within and across disciplines is highlighted as a prerequisite for understanding the development of the child. (GO)

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RESEARCH DIRECTIONS

in Child Development

synthesized by Joseph J. Sparling
James J. Gallagher
University of North Carolina

This booklet is a composite response of the following people to pertinent issues in the child development research field:

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A pre-publication draft of the text was circulated to each person to elicit comments concerning its accuracy in reporting on the discussions. Editorial changes were made accordingly. Although the text generally represents consensus, it also includes individual viewpoints not necessarily endorsed by the entire group.

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RESEARCH DIRECTIONS FOR THE 70's IN CHILD DEVELOPMENT

A report to

NATIONAL INSTITUTE OF CHILD HEALTH
AND HUMAN DEVELOPMENT
OFFICE OF CHILD DEVELOPMENT
OFFICE OF EDUCATION

from

FRANK PORTER GRAHAM
CHILD DEVELOPMENT CENTER
University of North Carolina
Chapel Hill

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The synthesizers of any conference must bear special responsibilities. The first of these is *coherence*. Discussion during the conferences was generally free and unstructured, so that the "pulling together" of such interaction required special effort. The synthesizers in this case had the professional support of Mrs. Judy Hulka who provided needed and valuable editorial assistance.

A second responsibility is that of *faithfulness* to the ideas presented by the various conference members. In this task we had the enthusiastic support and cooperation of the conference members themselves. Seventeen of the original panelists responded with comments and suggestions about the material in the report. These comments played a large part in shaping of the final draft, although the finished product has to remain the responsibility of the synthesizers.

The final responsibility is that of *modesty*. In a very real sense the compilers' major responsibility is to create a passageway by which the ideas, concepts, and suggestions of the conference members find their way to the reader. If there is insight in the report it is because the conference participants were insightful. We hope that the suggestions and ideas herein receive the attention that the importance of the topic deserves.

J. Sparling
J. Gallagher

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INTRODUCTION

In March 1971, the Frank Porter Graham Child Development Center held a series of conferences on child development research. Twenty-two distinguished scientists from across the nation met in three working panels, one each focusing on infancy, the preschool child, and the school age child. Their collective task was to assess the current status of the child development research field and to project research needs for the coming decade. This included identifying areas neglected by research, pinpointing obstacles to research, and suggesting solutions to research problems. A special focus of the discussions involved the policies of funding agencies and their impact on the child development field.

The conferences were convened on the premise that major changes are taking place in the research world which should be recognized, evaluated, and directed toward a productive end. The trend from individual to group research, from isolated research projects to a wide variety of development and demonstration efforts, and trends toward establishing priorities and escalating causes have altered the nature of research activity. These changes support an underlying belief that the results of research should have a more meaningful impact on the social condition. With this in mind, closer attention should be paid to how decisions are made with regard to the support of research activities.

Three agencies concerned with these issues, the **National Institute of Child Health and Human Development (NICHD)**, the **Office of Child Development (OCD)**, and the **Office of Education (OE)**, provided the financial support necessary to convene the panels and to report on deliberations. This text is submitted to the supporting agencies and, in a broader sense, is directed to policy makers, to the research community, and to the larger body of research consumers.

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FOREWORD

Several different and sometimes novel approaches to research decision-making and management in the 1970's were discussed at the conferences on which this booklet is based. Suggestions for change focus on the areas of:

- *priority setting*
- *synthesis of ideas*
- *communications systems*
- *new types of personnel*

In the area of priorities, formation of a National Child Development Planning Board might provide means for directing research toward meaningful social goals. The Board must be a freestanding organization whose members are charged with maintaining constant communication with the field's various publics. Such a National Planning Board would help determine and stabilize priorities by voicing the collective judgment of researchers and consumers.

The need to synthesize the results of research, within and across disciplines, was highlighted at the conferences as a necessity for understanding the child's development. There was consensus that some problems are beyond the scope of the individual researcher and require collaborative study. Ways to facilitate synthesis of research and encourage collaborative efforts are discussed in the text.

The panelists also concluded that there should be a communications system for research ideas and products. To achieve this, research needs new types of personnel, such as information specialists and synthesizers. The text defines the roles of these new workers in the research "family," along with suggesting how to recruit and support them. Comments on additional types of personnel needed to "round out" the research field are included in a section on manpower.

Numerous issues like the ones above are discussed in this booklet. The beginning pages identify the observed status quo in research. Suggestions for improvement are developed throughout the remainder of the text.

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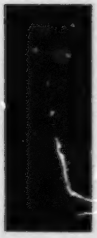
Current Trends in Research

What are the major research directions that you see the field of child development taking in the next decade based on current trends? As panel members responded to this question from their varied points of view, they created a composite statement identifying a number of trends in the following *areas* of the child development field:



- *learning*
- *cognitive domain*
- *social systems*
- *biological organizers of behavior*
- *linguistic development*

Within each of these broad areas some topics are currently being researched, others are just beginning to be studied, while many are not being investigated and represent gaps, discussed on page 7. Trends identified here refer to ongoing research as well as short range projections for future research based on what is happening now.

THE LEARNING PROCESS

A variety of topics within the area of learning have received and are receiving much attention. Research has become sophisticated in the measurement of various aspects of children's learning behavior, and has worked at refining the measurement of stimulus conditions that influence learning of children. The next ten years should produce progress in our understanding of

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very specific learning in cognitive and perceptual processes in infants and young children. In addition, it should provide important clues as to the manner in which individual differences in early learning experiences influence later behavior patterns—intellectual, social, emotional, cultural, and so on.

Another significant trend in the area of learning involves research to determine how undesirable behavior can be modified or shaped. This includes straightforward studies to expand the list of responses that can be brought under control, and the effect of "increased" stimulation on such responses of the infant as visual regard, reaching, vocalizations, developmental status, as well as test performance of the older child. Under this heading fall basic research activities associated with the "enrichment" studies, and the amelioration of the educational deficits associated with poverty. Finally, there will probably continue to be a proliferation of sophisticated operant studies.

WITHIN THE COGNITIVE DOMAIN

The panels identified an increasing tendency to expand the definition of cognitive behaviors as a trend which includes an effort to integrate variables into a common model with cognition. As investigations continue, the concept of cognition will be broadened to include such variables as language, judgment, creativity, motivation and the like. Cognition may become an extreme superordinate concept. We may develop a broad conception of intellectual functioning with particular emphasis on a number of subcategories which are currently growing in interest—for example, symbolic thought, and developing of symbol systems.

Another attempt to expand the cognitive domain is being made as researchers place more emphasis on the analysis of cognitive processes and competencies as these are manifested in the context of real life situations like the classroom or the play area. In other words, researchers show more concern for the nature of the demands that are made on children cognitively by different situations rather than stressing primarily laboratory type analyses of cognition and language. This movement will provide a broader scope in the search for the underlying cognitive processes which facilitate various kinds of adaptive learning.

There is a trend toward searching for the precursors of cognition in early infancy. This trend is regarded by some as the beginning of a larger effort to establish ties between early behavior and later behavior—seeking explanations in early childhood for future forms of development.

A second aspect of this trend includes normative studies of infant response to stimulation affecting sensory systems and varying in such dimensions as novelty. In this category, studies of the orienting response and of habituation are ongoing. If present practices provide a useful guide, investigations into the visual attention and perception of infants will continue to emphasize the stimulus, and studies of auditory perception will continue to emphasize the response.

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WITHIN SOCIAL SYSTEMS

Within the context of social systems, studies of family factors and parent-child interaction represent a current trend. These include measurement of parent behavior, correlations with concurrent and subsequent child behaviors, and modification of parental behavior to increase the child's test performance. There has also been progress in the study of dyads, such as the mother-child relationship. Panelists expressed the hope that in the future these studies will be replaced by research of more complex systems, of family interrelationships, and studies of social networks. Research within the area of social systems is also taking place on various social issues such as racial integration—a subject which will most likely continue to receive attention in coming years.

In the social realm there is also emphasis being placed on conducting impact studies related to specific attempts at intervention, such as Head Start. It is predicted that increasing research effort will be put into the analysis of the impact which the family, the broader society, and the community can have on children's development. More intervention activity will be aimed in the direction of influencing these aspects of the child's environment rather than concentrating, as is traditional, on the classroom or day care center situation. Panelists were hopeful that the trend will direct researchers away from looking for one best educational model for intervention programs, and toward looking for alternate types of intervention programs or educational programs which may be appropriate for particular kinds of children or teachers.

BIOLOGICAL ORGANIZERS OF BEHAVIOR

In an attempt to pin down the role of biological organizers of behavior, a trend is being established in the study of the postnatal period through the first 12 months of life. There is effort being made to link development to a variety of biological determinants or correlates of behavior, with special interest in the impact on development of early variations in conditions of pregnancy and birth. These include prematurity, low birth weight, nutritional factors, etc.

There is some contention that those concerned with human behavior must be concerned with the first three years of life in order to learn how to prevent and modify behavior which might result in later pathology. Insufficient or incorrect knowledge regarding the biological organizers of early stages of behavior can complicate later behavior, and lock in place poor problem-solving techniques, poor self-concept, etc. The trend toward study of the postnatal period through the first 12 months of life is related to these assumptions and has resulted in many young people with doctorates specializing in infancy. More studies in this area will involve psychopharmocology, biochemistry and nutrition.

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LINGUISTIC DEVELOPMENT

Practically every other developmental problem studied can draw from and contribute to the understanding of speech production, speech assimilation, and language learning. The increasing importance of language formation as a specific behavioral target for study constitutes a far-reaching current trend. Some researchers find this subject area enormously appealing, perhaps because it "has everything." For example, it is not there at birth, it is one of the first functions hit by brain damage, it is heavily influenced by environmental factors (the language used, the dialect spoken, the conceptual sophistication displayed), it relates as highly as any other human attribute to tested intelligence, it is clearly a product of imitation, it is used to make people angry or submissive or loving or anesthetic (as in hypnosis), and there is an enormous amount of descriptive linguistic data already available.

A TREND AFFECTING ALL TRENDS

In relation to all trends identified by the panels, members observed a current shift in emphasis from basic to applied research or development. It was noted that even basic research is beginning to occur more often in natural settings. The increasing frequency of such activities as the Educational Testing Service evaluation of Sesame Street and the Head Start studies supports this viewpoint. There will probably be continued emphasis on educational or quasi-educational interventions in the lives of various types of disadvantaged children, and particularly very young children or infants. The explosion of day care settings will undoubtedly figure prominently in such research. Researchers may, however, become broader and more humanistic in their definition of educational objectives, and correspondingly more flexible and nondoctrinaire in their use of program assessment.

As one researcher reminded the group, the best prediction for future research is more of what is going on in current research. The question brought into focus by this analysis of current trends is: will the directions we have been establishing take us to our desired goals?

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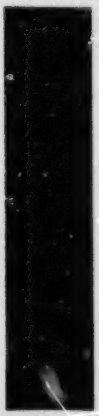
Gaps in Research Activity

In order to seek out the potential weaknesses in our current research trends, the panel explored the question: are there important areas or problem dimensions that are largely being ignored? The composite response which follows identifies areas in research which need to be filled in. Each of the specific gaps in research can be listed under at least one of four categories of *major gaps:*



1. lack of research on various *developmental periods,* such as the periods from **one to three years** and **20 to 29 years** of age;
2. gaps in the overall *content* of knowledge produced by research, with investigation being particularly limited in content areas such as the study of **values, affective domain,** and **social systems;**
3. too little *translation* of theory and knowledge into action, which fact severely limits the scope of **program development, instrument development,** and inhibits the undertaking of selected **intervention studies;**
4. too limited *synthesis* of research findings, which impedes **theory development** and is due to too little **conceptual integration,** and too little use of techniques which require sustained cooperation (collaborative studies and longitudinal studies).

A GAP: DEVELOPMENTAL PERIODS

The period **below three years of age** can be singled out for special research emphasis. Not only do we need theories which apply to this age span, but detailed ethological and ecological information are necessary. In addition, information about interaction between experience and development must be

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generated from direct and suitable experimental work. This research gap is especially evidenced in the lack of study of the developing female infant and child.

A concern for children also requires more study of adults, so that a life span developmental psychology can evolve. We still need to know more about the **ages 20-29 years**, the age period which especially represents parents of young children. There is much work to be done in tracing behaviors through major periods of development, fitting together data from studies of different stages of development, and relating particular periods of life to the whole life span. A more complete discussion of developmental periods is included in the section on collaborative studies, page 28.

The sequence of research is often seen as natural observation, hypothesis building and experimentation. Although the entire sequence is needed, natural observation is often missing. A more complete natural history for all age periods, but especially those mentioned earlier, would provide needed information such as which variables are gradual, quantitative, and continuous as well as which variables are uneven and discontinuous. The study of normal and especailly bright children is likely to be productive in the natural history area. Another productive approach would be ecological study utilizing the total natural setting as it exists rather than to institute interventions and to study these. Simply stated, we will need knowledge in detail of how children feel and what they do over extended periods of time when engaged in learning.

A GAP: CONTENT AREAS

Gaps in the **affective domain** need immediate research attention. For example, facts are needed on the stability and consistency of affective variables through various developmental periods. More research attention should be focused on esthetic development, and existing knowledge needs to be applied to problems of ego and emotional development.

Study is also needed on the consistency of **values** and moral character traits within children. In addition, we need to understand value systems, belief systems, moral and ethical development. Research on educational goals and values in society is a difficult but urgent task.

Finally, there is an important need for research on complex **social systems.** One of the initial tasks in this area is the study of the effect of social stimulations on children. While the mother-child relationship has received much attention, the response of the child to the father and later to peers represents a research gap. Beyond the study of social dyads there is the need for the study of triads and more complex social systems. From the perspective of a researcher working in the schools, omissions in the area of social systems are seen as a vacuum in which we are treating children. There often seems to be no awareness of the parents and little awareness of who is going to be working with children—paraprofessionals, teenagers, etc.

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A GAP: TRANSLATION OF RESEARCH FINDINGS

There is extremely little translation of research findings into **program development** or curriculum development. Taken in its broadest sense, development includes not only program development but program evaluation and dissemination. The development problem is a new one. The Office of Education (OE) spends about one dollar on development to every one dollar spent on research. It was suggested that ten to one is a realistic ratio of development dollars to research dollars. Much of the OE research is somewhere down the road toward development and probably should cost at least five times as much as basic research. A problem is that research and development are not always completely distinct from each other; they exist on a continuum.

Development activities are costly because of their complexity and scope. Not only do they require the production of materials, field evaluation, dissemination and related training, but also large staffs and continuously available child populations. However, this complex chain of activities has as its payoff the integration of child development research with social problems. We need, for example, to integrate the information on the first few months of life and apply this to the problems and issues of infant care. Other solutions to problems find their application in home and school curricula.

Intervention studies represent another opportunity for the translation of basic research knowledge. The gap in this area is hardly the result of too few studies! In fact, gross intervention efforts have been too numerous. Some ground rules are needed to assure that new ventures in this area will provide outcomes beyond the knowledge now reasonably well established. Such established knowledge can be summarized in three points:

1. The developmental patterns of deprived children can be modestly accelerated under major and systematic programs of stimulation.
2. When the stimulation program is removed, the youngsters will lose a significant proportion of the gains made under the special program conditions.
3. The youngsters who respond best to a program are those who have fewer negative factors in their environment to begin with; that is, not emotionally disturbed, no family disintegration, etc.

We should demand that future intervention studies have ambitions to do more than merely redundantly state the above findings. Future intervention studies should focus on needs such as these:

- *the effects of family-centered intervention as contrasted to day care and nursery school*
- *upward age extensions*
- *more power in intervention programs*

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[redacted]

. *more genuine variation in the features of in-school intervention*

. *study of the heterogeneity of disadvantaged groups*

The need to contrast family-centered intervention with child care in group settings is a pressing one since it appears likely that, in the future, an increasing number of educational activities can and will occur within the home. Many parents will be able to choose education in the home setting as an alternative to group day care. Research needs to compare and make known the effects of these various types of intervention.

Upward age extensions, or programs for older children to support the positive results of earlier programs, are needed. The familiar wash out of positive results should not be accepted as inevitable until programs covering longer time spans are implemented. Research also needs follow-up programs to check "sleeper effects," especially those negative responses which don't make themselves known until after a child leaves the program which evoked the response in him.

Most current intervention studies represent a very small input in relation to the total life experience of children. It is not surprising, then, that programs create small effects in children which are lost over time. To put more power in intervention studies, the number of hours a child spends in a program could be increased or the program content could be refined.

In addition, more genuine variation in the features of in-school intervention is needed because the current differences in input often prove to be minor. These differences are more often related to teacher differences than to differences in program models. Program models may provide the basis for real input differences, but models are often inadequately implemented. Programs should be designed to guarantee departures from the standard, often mediocre approach, and the results of these departures should be carefully measured.

It is quite clear that much more work needs to be done in understanding human variability when studying disadvantaged children. The "disadvantaged" have long been regarded by research as a monolithic group, when, in fact, there are significant differences between the urban and rural disadvantaged child and those considered "gifted." Research needs to focus study on the heterogeneity of disadvantaged individuals and groups.

In addition to program development and intervention studies, the lack of **instrument development** is included here as a significant and debilitating gap in the translation process. Almost every conceivable translation of basic research into action is dependent upon the available instruments of measurement. The instrumentation gap has such far-reaching effects that it compounds the gaps listed under categories other than translation: developmental periods, content areas, and synthesis.

The meager supply of instruments and methodology available in some areas may make the researcher feel that before he can build a house he has to first stop and make a hammer and some nails. Historically, certain instruments and tests were not developed as part of a research effort until there was a "big push" in the National Aeronautics and Space Administration. It fol-

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lows that a big push in child development may be necessary before needed instruments are developed.

Some specific instrument needs are described as: better assessment techniques for learning processes as opposed to learning products; instrumentation in the affective domain; early assessment of personality.

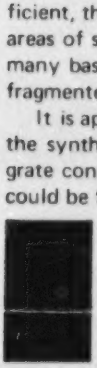
A GAP: SYNTHESIS OF KNOWLEDGE

Child development research experiences a major gap in **conceptual integration** and, specifically, in **theory development.** We need to assimilate much existing fragmentary knowledge, and generate theoretical models which specify critical parameters of child development. From these models we need to develop theory. Theory development is not a luxury in child development research, but a necessity. At present we have a base of knowledge which has perhaps too many major gaps to be a truly fertile ground for the development of theory. Without a firm knowledge base, the development of theory is difficult. Since theories are developed so that the world can be better understood, workable theories can have impact on social issues and actions. Without synthesis, without theories, it is almost impossible for child development research to keep up with social needs.

There are certain research styles which encourage conceptual integration and, therefore, theory development. Both collaborative studies and longitudinal studies are capable of facilitating synthesis and creating more comprehensive bodies of knowledge from which theories can emerge. An advantage unique to these kinds of research is that they provide more representative samples for study than are available to the individual investigator. Specific longitudinal studies are currently needed in such areas as affective development of children, especially across social classes. Such sustained efforts have certain problems inherent in them. Possible solutions to some problems are examined under "The Need to Collaborate," beginning on page 28.

To encourage collaborative and longitudinal studies is not to discourage basic research. Basic research is and should be a part of any total research effort. Because basic research has received some level of support, albeit insufficient, there is a tendency to focus on other more deprived areas, particularly areas of social sensitivity. The gap – synthesis of knowledge – suggests that many basic studies, although essential to the total research enterprise, exist as fragmented, isolated projects that need to be linked together.

It is apparent that there is a need to create a research environment wherein the synthesis of knowledge is encouraged. Researchers should strive to integrate concepts and build theories. Panelists contend that theory development could be fostered by:



- *stabilizing priorities so that funds would be available for longer periods of time*
- *altering the reward system to credit the developer of theory rather than dissuade him*
- *encouraging collaborative study*

These suggestions for nurturing synthesis – filling the gap – are elaborated on in upcoming sections of this report.

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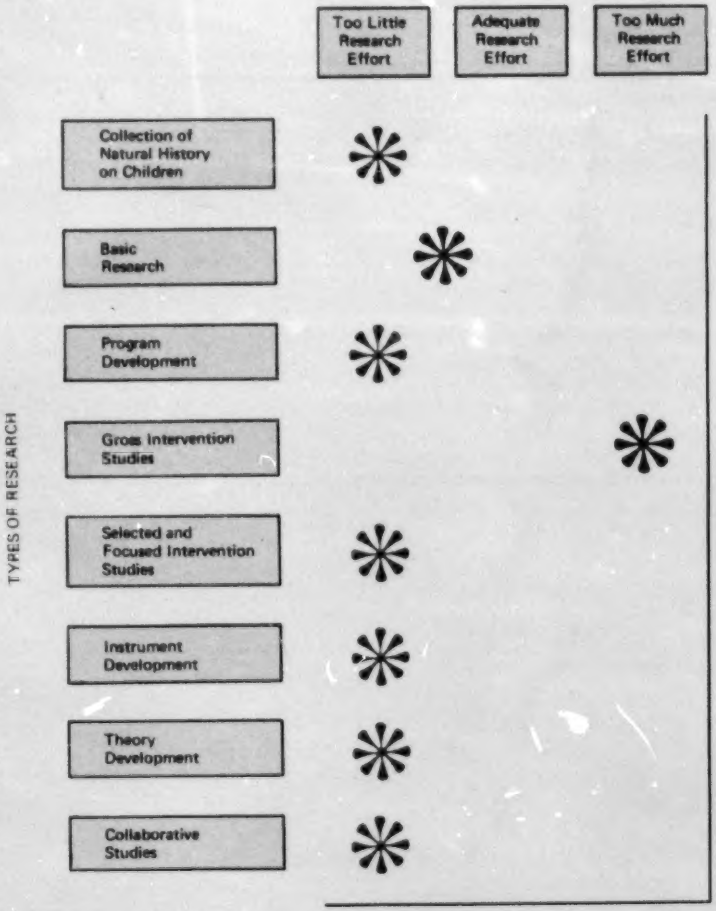
The following chart is a summary judgment on research emphasis in child development today. It not only attempts to show in which areas there is too little research, but identifies areas wherein adequate or too much research is being conducted.



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Barriers to Research Efforts

In response to the discussion of research gaps, the panels identified a number of barriers which have created and are perpetuating these gaps in research activity. They are in the form of implicit or explicit policies of institutions responsible for the support of research. There are several reasons why many research problems are not studied, but the most significant of these *barriers* are:

1. *policies of the university and profession*—these involve the nature of the **academic reward system** and those factors related to professional and peer status, in addition to policies which encourage **isolation of researchers** in their work;
2. *policies of government funding agencies*—these range from **excessive reliance upon unsolicited proposals**, to policies of **short-term funding, attitudes toward instrument development** and **population availability**, and the general **lack of funds.**

These policies or barriers demand our special attention because they constitute one of the most likely areas in the system into which change may be introduced. Few of the policies which act as barriers are formal policies. Instead, they represent habitual ways of acting which serve to create de facto policies. Both the funding agencies and the academic profession have strong de facto policies which appear to perpetuate existing research gaps.

BARRIERS: POLICIES OF THE PROFESSION

The research scientists' **reward system** acts as a barrier to certain types of research. For example, the longitudinal study of young children may carry a professional reward which is too distant. Quick publication of the short-term research project provides the clearest opportunity for advancement up the

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academic ladder in the university setting. As long as we are tied to a "publish or perish" philosophy, we will not have an environment for long-term attack on difficult problems. Other areas in which gaps exist (gathering of natural history, instrument development, or synthesis of research findings into theory) also require lengthy study, and this kind of effort is not adequately or quickly rewarded by the profession or the university.

The reward system barrier perpetuates research gaps which in turn act as barriers to other kinds of research. This chaining of events magnifies the significance of the reward system barrier. For example, the reward system does not especially encourage theory development. Because the development of theory may be a long process, the researcher involved may not be in a position to publish his findings and receive recognition until many years after his colleagues publish short-term research in other areas. As a result, many researchers shun this kind of effort and a gap exists in theory development. The gap in theory acts as a barrier to the research activity of instrument development. The gaps in theory and instrumentation in turn inhibit the study of certain content areas such as the investigation of social systems. Thus the chain of cause and effect reaches from the reward system, to theory development, to instrument development, and to the study of such complex areas as social systems.

In addition to the reward system, a second research barrier is the relative **isolation** in which researchers function in the university setting. Policies of the university make an independent, solitary approach to research the most likely to pay off for the individual. The professor typically finds himself in a university department made up of individuals selected for their different interests and capabilities rather than similarities in terms of teaching experience and research interests. As would be expected, this results in many small research studies on a diversity of topics. While this organizational pattern produces an attractive and diversified research output for the department, and an opportunity for each individual to progress at his own rate in "making a name for himself," it makes unlikely the occurrence of other more cumbersome research activities, such as collaborative studies. In the opinion of the panels, collaborative studies must be encouraged if certain complex questions in child development are to be answered. This view is supported in a special section on collaborative effort.

In summary, the de facto policies of the university and profession can directly act as barriers to the filling of research gaps in the areas of translation and synthesis and, indirectly, in the areas of content and developmental periods. The *policies* referred to here can be stated briefly as follows:

- *The reward system within the academic profession implicitly encourages short-term studies and discourages studies requiring sustained and/or cooperative effort.*

- *Isolation produced by university organizational patterns creates a tendency among researchers to undertake investigations by themselves, limiting findings to the realm of their own skills and capabilities.*

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BARRIERS: POLICIES OF FUNDING AGENCIES

Certain types of research are made very difficult by the policies of funding agencies. Many of these are de facto policies and may reflect certain procedures of government not under direct control of the funding agency itself (i.e. annual appropriations by the Congress). However these policies may have developed, they are responsive to, and thus reinforce the weaknesses in the academic situation.

An example is **short-term funding** which offers little hope that particular areas of research can be pursued to completion. This agency policy acts as a barrier to theory development. Developing theory takes time and the need for time implies the need for long-term funds. In fact, the particular gap of theory development is probably one result of short-term funding policies. Other research efforts which are inhibited by the policy of short-term funding are program or curriculum development, certain intervention studies, and instrument development. Such policies also severely limit performance of people at research centers who want to attack major problems but are hard put to get long-term commitments.

Short-term funding is related to short-term agency priorities and an **excessive reliance on unsolicited proposals.** Presently, most agencies select studies to be funded from randomly submitted research proposals. This laissez-faire approach to determining how grant money will be spent belies overall planning in the research field. A continually shifting set of funding agency priorities may act as an imposing barrier to closing the critical gaps in our research activities. The effective managing of research priorities is, in fact, an area of such importance that an entire section of this report is devoted to it (see page 18).

Agency **attitude toward instrument development** accounts in part for the lack of activity in developing techniques of measurement and other research methodologies. Individuals have received word from particular agencies that instrument development must be cut out of a proposal before it is considered acceptable. This fact perpetuates the gap in translation of research findings into usable programs.

An area in which funding agencies share a policy stance with universities is in their **attitude toward carrier populations.** A carrier population is one which is receiving program services over an extended period of time primarily to allow the population to become available for research purposes. To create carrier populations we must create and support sufficient services to encourage parents to have their children and themselves participate in research efforts. Currently, the cost of maintaining a population of preschool age children for study presents a large obstacle and, therefore, agencies are reluctant to support study populations. Even though certain types of research require ongoing carrier populations (i.e. curriculum research, longitudinal studies, etc.) the fact is not squarely faced by most universities or funding agencies.

This attitude toward carrier populations is related to the research gaps which exist in various developmental periods. For example, populations of children under three years of age are generally unavailable for study over

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extended periods of time. As a result, researchers know little about this age group. Cultural attitudes such as the sanctity of the home are partially responsible for the past unavailability of those under three, but these attitudes are changing. Parents now often consider it acceptable to secure services for infants and toddlers. Once a service like day care is provided, this population of children becomes available for possible research.

In addition to examining agency attitudes toward supporting carrier populations specifically to carry on research, the idea of "piggy-backing" research on already existing service operations should be examined. Whenever and wherever group settings exist, the possibility of supporting research populations in these settings should be seriously considered. For example, Dr. X who needs to observe the eating habits of three year olds over an extended period of time might be able to work out a contractual agreement to do so with a day care center where this population is available. Such possibilities of piggy-backing research are seldom explored, even though the concept suggests efficient use of already existing resources and has an economic advantage over establishing new carrier populations. Population availability is vital to many studies.

A final reality of funding (which can hardly be called a policy) is sheer **lack of funds**. A comparison of the total resources for child development research, for example, with agricultural research or medical research reveals one of the basic roots of the total problem. The lack of funds has a direct or indirect effect on every research gap listed by the panels. Some of the specific hardships related to limited funds are seen in restrictive guidelines, unstable and late budgets, unrealistic deadlines, a lack of effective planning between agencies, and reluctance to invest sizeable sums in long-term projects.

In summary, the altering of funding agency policies could remove many existing barriers in the child development research field. In regard to these de facto policies it can be said that:

- *Short-term funding is a reality inhibiting the completion of many studies. Agencies presently place little importance on the synthesis of knowledge, as is demonstrated by current unwillingness to extend funds for longer periods of time.*

- *Excessive reliance on unsolicited proposals in deciding how to distribute money belies the systematic development of agency priorities, and prevents study of some significant areas.*

- *Instrument development is discouraged because costs for it are often left out of research grants, which fact inhibits the translation of research findings into usable programs.*

- *The need for carrier populations is not recognized by agencies who remain unwilling to fund them exclusively for research purposes.*

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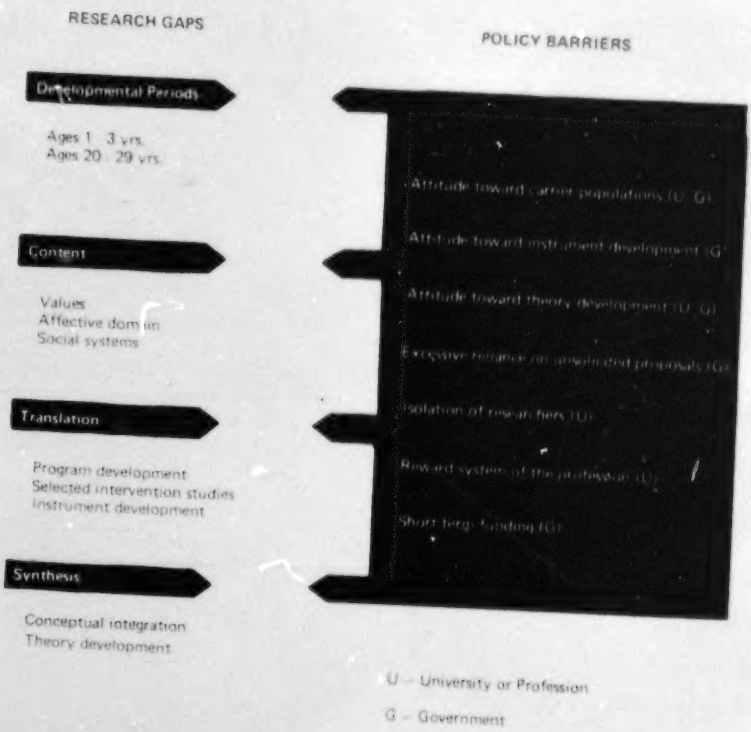
Each policy barrier is responsible for research gaps previously discussed. Almost every barrier can be related to every gap. A summary of their relationships is seen on the following chart.

BARRIERS THAT CREATE
AND SUSTAIN RESEARCH GAPS



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In the preceding sections, panel members identified trends, gaps, and barriers that presently exist in child development research. In the discussion which follows, a number of specific recommendations are made based on careful assessment of what was observed as the status quo.

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A Move to Set Priorities

There appears to be a strong trend towards the establishing of priorities in child development research. Some researchers, in fact, see not a trend but a tidal wave in that direction. This approach is not intended to totally supplant the laissez-faire method of relying on researcher-initiated proposals. It is intended to supplement the laissez-faire method with stronger and more consistent agency-directed priorities.

Several mechanisms are presently working toward the systematic establishment of priorities. One occurs when consumers, allied with scientists, attempt to influence legislation and, therefore, agency policy. Another occurs when government officials responding to changing administrations feel the need to state priorities which have not been in vogue. It is this continual changing of priorities which gives many researchers the idea that the federal government has a propensity to manufacture research priorities, and which gives rise to researchers' fear of agency-set priorities. However, these very scientists who make up the field of child development have demonstrated clearly that they, as a collective group, have no capacity for organizing their own research efforts in a coherent and productive fashion. This rather confusing situation points to a key question: how should a priority-setting effort be managed?

A possible answer to this question is that the funding agencies should do so but only after having been informed by a widened input from both consumers and researchers. Consumers should have a voice in deciding which social problems should be attacked by program developers and disseminators. If there is considerable public interest in making social advances that await scientific solution, the funding powers should be responsive to the need. Another answer is that researchers should speak through a central group which is informed by a poll of scientists. This group would be more than just a lobby for scientists. It might organize regional and national research conferences to develop and state priorities, perhaps taking the form of a National Child Development Planning Board.

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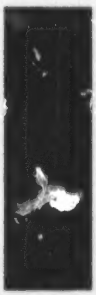
NATIONAL PLANNING BOARD

The *purpose* of a National Child Development Planning Board would be to help set priorities, stabilize priorities, determine gaps in research activity, and overcome barriers to research. The main *functions* of the Board would be to:

- determine major research and development needs
- develop lines of alternative strategies to meet needs
- serve as a communications channel between government, research, the public
- determine manpower needs in child development research

The following summary of *characteristics* helps to describe such a National Board. It should:



- be a freestanding organization, not part of any federal agency
- be heterogeneous (made up of consumers, researchers and government representatives)
- be an eyeball-to-eyeball operation (involve direct consultation with both agencies and researchers)
- have the ability to gather information
- have the ability to represent and communicate to agencies major consensus and minority points of view
- not be a group that sponsors its own research
- have built-in rotation elements in terms of membership

One of the subcommittees of the National Research Council (NRC), the operating arm of the National Academy of Sciences, might aid in establishing such a Board, or the Center for the Study of Democratic Institutions might provide help in thinking through the problems involved in the formation of such a group.

The Board could be formed in association with a consortium of life insurance companies interested in enhancing the quality of life. Support through such a likely component of the free enterprise system would assure a freestanding Board, one without need for public monies and, therefore, less inhibited in matters to which it would address itself. Such a consortium could give financial and moral support to a Child Development Planning Board, provided the organizations do not insist on high profile involvement as a profit motive. There would be at least two advantages to the life insurance idea: the Board would gain accountability to stockholders; the Board would gain actuarial talent which might be used to compute the probability of payoff in various research endeavors. Another alternative for forming and maintaining a National Planning Board would be for all related professional organizations to contribute a small amount to the Board.

These suggestions merely represent the breadth of thinking on the topic, and are not meant as specific recommendations for support of a National Planning Board.

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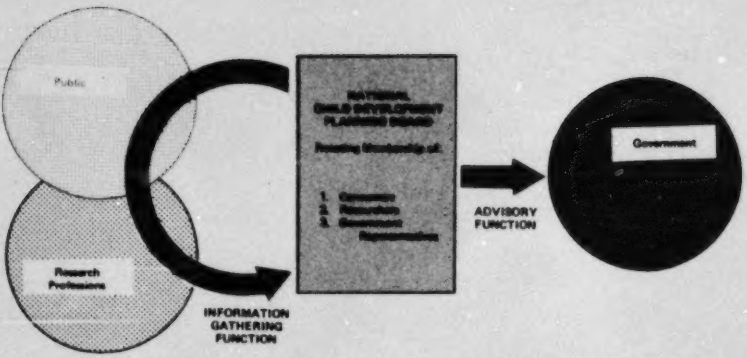

STABILIZING RESEARCH PRIORITIES

Vital to the completion of numerous research studies is the ability of the researcher to protect himself against the disease of *governmental amnesia.* It is the consensus of panel members that this malady continues to create havoc in the child development field. According to a recent article in *Science,* research on a new topic should be supported for 10 to 15 years after a few initial breakthroughs have occurred. Presently, funding agencies establish priorities, provide the money for research into priority areas, then in a short time "forget" that they once considered the area of study significant. When this *amnesia* occurs, often within two to three years, priorities slip into disfavor and funding for them is tapered down or entirely cut off.

Because it is a waste of time and money to only partially complete research activity, the panel suggests that positive efforts be made to stabilize priorities. Stabilization could be encouraged by:

- *funding fewer projects but more completely*
- *improving communications with key government policy makers so that funding agencies would learn to appreciate process research rather than only the products of research studies*
- *setting aside 20 percent of the funding agency budget for long-term commitments*

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In a brainstorming fashion, a panel member created the following chain of ideas to form one perspective on how stabilization might be fostered:

I. Move some priority setting out of the hands of the government by

   A. offering tax advantages to philanthropic foundations which give long-term support to significant lines of inquiry;
   B. giving support in renewable five year blocks to productive individuals;
   C. providing endowed research chairs at universities.

II. Encourage increased participation in priority setting by people inside and outside government by

   A. creating a new federal/state research and development (R&D) plan with jointly set priorities (to stabilize as well as to build a politically-strong constituency);
   B. having proposed priorities reviewed by
      1. federal government representatives,
      2. private advisory groups,
      3. prospective users of the results.

III. Associate priorities with stable programs by

   A. allocating R & D funds for all operating programs;
   B. having operating bureaus participate in setting R & D priorities;
   C. financing R & D institutions so that they can add service units (which operate at a profit) to draw R & D funds from clients, and committing each such institution to continue the R & D lines which prove profitable;
   D. establishing long-term R & D training programs with large institutional assistance grants sufficient to support considerable research along with conducting training;
   E. converting all Title I evaluation funds to research and evaluation dollars, and considering strategies such as cross-project research with process monitoring in individual projects.

IV. Target R & D, specifically D, and associate funds with an expected project to be delivered on a schedule by building product descriptions and timetables into the statutes themselves.

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HOW TO JUDGE PRIORITIES

It is important to establish guidelines for judging which problem areas should be given high priority. Points to be considered are:

- *seriousness of the knowledge gap*
- *the social need (priorities should be related to the goals of society)*
- *probability of payoff in the area*

Panelists agreed that a rational diversity of priorities must be maintained. Within that context, they strongly emphasized that non-priority research—that involving the beginner, the "maverick" researcher, the person doing "small" studies, or work on a "cold" project—must continue to be funded with a certain amount of dollars. One suggestion was that 10 percent of funding be allotted to these kinds of investigators since their work would most likely be research; less expensive to conduct than development. The remaining 90 percent would be focused on a variety of efforts in priority areas. The problem aptly noted by the conferees is that neither the 10 percent for basic research, nor the 90 percent for other development activities is enough. There is a lack of total dollars and, to be frank, a lack of confidence in researchers' ability to aid in helping solve society's major problems.

Regardless of the percentage breakdown of funds, all panelists contend that the laissez-faire approach to selection of research topics has its merits and should be preserved as part of the total research effort. One reason for this is that a priority list which excludes certain study areas would soon cause gaps in those areas and they would eventually become priorities. In addition, there is no way of knowing what priorities will be in the future. For these reasons, it is important to preserve the laissez-faire approach which permits the researcher to choose his own area to study. Coupled with priority research, such investigations would assure that a wide range of subjects was being studied.

In the history of research, the mission-oriented approach of the Manhattan Project and recent targeted research developed by OE provide examples to be studied. R & D centers and Regional Education Laboratories which could be organizations for targeted research have been criticized, however, as examples of a "grand design that doesn't work."

New research into past methods employed in priority setting, such as a study of what efforts have succeeded and failed in R & D centers, etc., could be helpful in solving priority problems. It is reasonable that we use research to solve research problems by delving into "priority literature."

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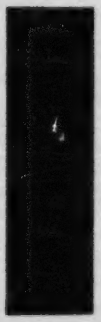
Another point to consider carefully is the way in which research relevant to priorities should be reviewed. Some current priority areas are made known by agencies only a short time before the deadline date for submitting proposals. Even then, notice sometimes goes only to a few research groups. As a result, very few proposals are submitted. Those submitted are usually subjected to peer review by an ad hoc committee. Some panel members felt that that kind of review is not as stringent or as thorough as review of unsolicited proposals. It was thought that many proposals accepted by ad hoc committees would not survive the competition of a regular study section.

If priority areas are to receive greater emphasis in the future, announcements for the solicitation of contracts must be made to the scientific community in general. They must also be made with sufficient *lead time* to permit the development of respectable proposals. These proposals should be subject to review by a regularly constituted section considering a number of proposals of a particular substantive type. Suggestions by an ad hoc committee and monitoring of research by a section of a funding agency are not adequate substitutes for open competition of research proposals, or for review of them in a broader context.

GETTING PRIORITY WORK DONE

The critical question to face is: how does needed research get done? There are those people on the frontier who are high risk people, but then it is necessary to have the settlers who come after. This is the area in which incentives are needed.

Some mechanisms or incentives for attracting individuals or organizations to pursue high priority research might be to:



- *make established priorities highly visible in order to draw research proposals*
- *offer professional awards for work in priority areas*
- *invite bright researchers to attend conferences outside their specific content areas (they just might get interested in the topic)*
- *grant funds to allow an additional year on a dissertation, if the subject being researched is a priority subject, in order to expand the thesis into a truly worthwhile study*

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The Need to Communicate

The fact is clear that there should be **more** and **better** communication in the research and development area. Answers regarding to whom such improved efforts should be directed and how to go about communicating are not quite as obvious. There are two distinct *channels* in which communications should flow:

1. There needs to be an *information transfer* between researchers themselves as well as between researchers and their publics (diagram, page 29).
2. There needs to be *program transfer* in the program development process (diagram, page 26). This second channel of communications would serve to translate research into programs of action and aid in implementing them.

Traditional customers of the researcher have been fellow researchers—colleagues interested in mutual disciplines. Disseminating results of one's study to another research-oriented investigator who can evaluate and react to it is more "second-nature" than trying to communicate in the language of the layman, the legislator, or even the funding agency. Although traditional communication remains important, there is a growing feeling of social responsibility on the part of the researcher to communicate with the mass media, the public—ultimately the consumer of his product.

On the other hand, without safeguards, the researcher's interest in communications could become a disservice to research itself. The problem may be more one of insulating active but poorly evaluated research from the public, from government, and from practitioners than one of finding more effective means of rushing research findings into general circulation.

Many of the findings reported on are premature, inconclusive, contradictory, at best suggestive, seldom definitive. Much of the information in them needs interpretation, integration, and translation, with special attention given to implications of policy or of practice. Such critical assessment should be

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made by other researchers who know the area of study. One of the safeguards of research would be not to disseminate research findings to the public until stable conclusions have been reached. Perhaps, results of a single study should not be made available to the public unless it summarizes or typifies a "line" of research for which there is consensus.

Most writing that has been done to date in the milieu of research has been geared to professional journals. Dissemination through that medium has been high, but the effectiveness of journal information in terms of *social goals* "hovers around zero." Journal articles were singled out by panel members as perhaps the "least effective form of communication known to man."

An important missing link in the communications chain is a free flow of information between research and development. Research findings should be transmitted to development people who, in turn, should transmit them to practitioners as validated instruments and programs. The diagram on the following page illustrates such a progression.

The idea of transmitting research information all the way along the continuum in unmodified form is not useful. There is an elaborate transformation from the beginning to the end of the process diagrammed. It is also unwise to assume that nothing can be done along the continuum until research has first broken ground. Results of existing studies can be implemented so that communications can be satisfactorily begun at any stage. For example, development work can be done, starting not from research evidence directly, but from the accumulated wisdom of practitioners.

Once research has been conducted, one should not expect that there will always be semi-automatic progress along the continuum which will result in product and practice. For one thing, in moving from left to right on the diagram, each activity involves larger numbers of people and resources which are not always available.

TRANSFER OF RESEARCH IDEAS AND PRODUCTS

There is a growing need for special mechanisms to aid in communications between the researcher and his publics. In order to communicate better, R & D should develop two new types of *job categories* within its ranks:

1. *synthesizers,* individuals who can correlate findings in major fields in an organized and systematic way so that knowledge can exist in a more usable and accessible form;
2. *information specialists,* individuals who can translate the language of the researcher into the language of the policy maker and the consumer.

Because the investigator's talents do not often coincide with the journalist's, *information specialists* are needed in the research field. Call them science writers, public information specialists, or public relations men, such people would represent a new breed in the child development family. Science writers

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TRANSLATION OF RESEARCH TO DEVELOPMENT TO ACTION

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are not new, but this type of information specialist would be new because he would be an advocate of the researcher and an integral part of the child development research system. To recruit him and train him seem challenge enough, but to pay him is an impossibility for many under present funding conditions. To help resolve this, panelists suggest that:

- *research be funded in such a fashion that dissemination costs are built in*
- *the position of information specialist be initiated with the inception of a National Child Development Planning Board (discussed on page 19), for which funding problems might be less acute than at other levels*

THE SYNTHESIZER AS COMMUNICATOR

In addition to information specialists, panel members suggest that the profession take a hard look at the need for *synthesizers*. Such individuals must necessarily be knowledgable in a number of areas and work within interdisciplinary environments. They would "make something" out of information secured in the last period of time, which often stands as isolated studies in interrelated but distinctly separate disciplines. Synthesizers could contribute to more efficiency in the development of products through research. Such scholars would represent the intermediate step between the research phase and the service phase – or last phase in the research-development progression.

The role of synthesizer has been experimented with to some extent in the Educational Resources Information Center (ERIC) with various problems emerging as to recruitment of personnel and financial support. Experience shows that it is difficult to get seasoned scholars to work at the task of synthesizing because they are often already committed to a line of research. It is also difficult to finance the training of such individuals unless training can take place in an already established setting. R & D centers where both theoretical and practical work is carried on in the ordinary course of events might be the best environment in which to train synthesizers.

To encourage researchers to become synthesizers, and to elevate the function of synthesis to an important role in the research field:

- *Offer grants to institutions willing to carry out organized and systematic synthesis in major fields.*
- *Hold symposia at national meetings on the results of synthesis.*
- *Encourage professional organizations to offer awards to outstanding researchers functioning as synthesizers.*
- *Train the new PhD in an interdisciplinary environment before he becomes committed to a line of research.*

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To decide who should be responsible for correlating individual studies into a unified synthesis of thought is another problem to consider. Ideally, synthesizing should be the responsibility of a disinterested group not unlike the proposed Planning Board. The fact remains that, regardless of who does it, synthesis is needed in research. The current research system belies the synthesizer function primarily because few researchers are trained in interdisciplinary work.

The challenge of communicating more extensively and in better ways is one to be met in the coming decade. There must be improved dissemination of information between researchers, researchers and their publics, and better means employed for communication between researchers and program developers. The recruitment and training of information specialists and individuals to synthesize research is significant to the overall effort to nurture research.

The following diagram integrates the suggested new personnel into the communications system which involves itself with the transfer of information between researchers themselves and between researchers and their publics.

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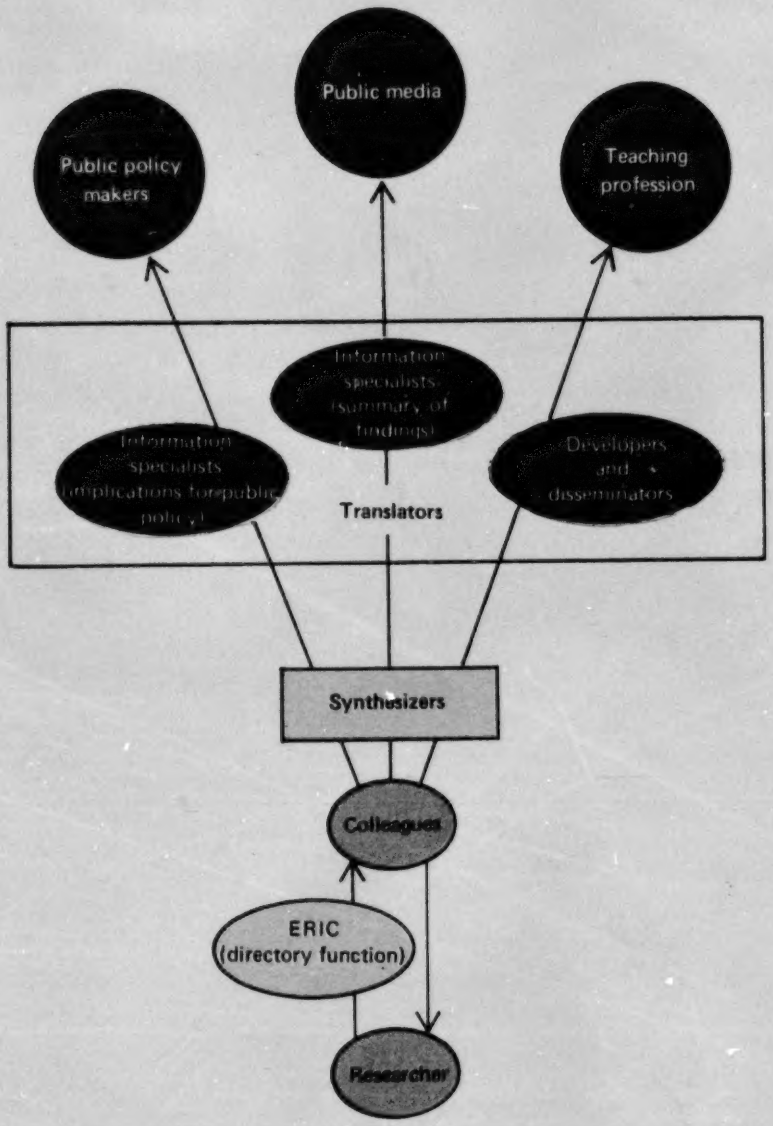
COMMUNICATIONS NEEDS



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The Need to Collaborate

There appears to be a strong sympathy for various kinds of cooperative efforts between research centers and between individual researchers. Research styles which rely on cooperation are the ones most capable of tackling the toughest problems—those problems which appear to be *beyond the scope of the individual researcher in terms of his financial and organizational resources and conceptual ability.* If these difficult problems are to be attacked, research and development teams should be the thing during the next decade. Many of the problems which we now face will require interdisciplinary work, multidisciplinary work, and cross-cultural study.

A particular research style which depends on cooperation is *collaborative study.* Collaborative research can exist on a number of different levels. These may range from complex interdisciplinary studies in which colleagues are dependent upon each other, to "parallel play" between researchers in which the same problem is studied in an individual but parallel fashion. An important argument for collaborative studies is based on the fact that, in the 1970's, social systems may be changing faster than researchers can develop stable findings on the small and parochial samples now used. By the time synthesis of fragmented, individual research takes place, the phenomena studied may have already changed into something else. If, under the present system of independent research, it remains unfeasible to reach stable conclusions at a faster pace, the research community will lessen its capacity for influencing society or for being relevant. Research will be unable to keep up with the rate of change. Collaboration among investigators could accelerate the process of reaching stable conclusions.

There is a question as to what kinds of research require collaborative effort. Some would involve the plotting of development in areas where disciplines overlap. For example, studies on the effects of nutrition in children might engage individuals in education, medicine, psychology, psychiatry, etc.

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Where differences in populations or physical environments have some relevancy to a study, collaboration could be almost imperative.

From the standpoint of research in general, however, it must be noted that collaboration for its own sake is not good. Only when a problem can be best attacked through collaborative effort should this kind of study be pursued.

PROBLEMS IN COLLABORATING

Efforts to collaborate have been relatively few in the past because of the problems inherent in the collaborative undertaking. One set of problems lies in the process of communicating, and can be divided into four major areas:

1. spatial organization;
2. professional conflicts;
3. locus and means of decision-making;
4. personality conflicts.

Too often researchers who need to exchange knowledge are in separate buildings, on a different campus or in another state. Their spatial organization is presently unconducive to collaborative study.

Professional conflicts can arise between collaborators concerning the focus and direction a study should take. These are often the result of distinct differences between: theoretical beliefs; study methodology; interpretations of findings; which target groups to communicate with; and who should receive credit for publication.

Another deterrent to collaboration, the locus and means of decision-making, is involved in establishing who has the final word in a joint effort. Even when this is made patently clear, a collaborator may begin to be uncooperative when one or more decisions conflict with his own views. He may then work only when required by contract, work at less than the normal rate, or even publish prematurely.

Personality conflicts can result when personal traits such as ambition, jealousy, distrustfulness of colleagues and their motives enter into a cooperative effort. Some people are not good team members. They may have trouble sharing their ideas, taking and following directions, and communicating openly with colleagues. Others may fail to see how their small contributions will be of any importance to a large project or how being involved in a group study will help them. Such considerations are important for the researcher as he sets his priorities.

Unfortunately, the problems noted above discourage researchers from cooperating with their colleagues. Failure to collaborate diminishes the probability of strengthening research through integrating the findings of two or more fields. It perpetuates the gap in synthesis of research.

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THE FUNDING DILEMMA

The dilemma of funding collaborative studies was mentioned earlier in the report as a barrier to developing inter- or multidisciplinary research. Some panel members felt that the difficulties in seeking funds within a single institution were so great that it seemed unlikely funds could be gotten under present arrangements for collaborative efforts between universities. In addition, collaborative research takes time, and agency policy of short-term funding discourages extended study. Panelists suggested that two types of grants be considered as *models* of the kind of funding needed to encourage collaboration:

- *the core development grant which is presently available to Mental Retardation Centers supported by NICHHD*
- *the biomedical sciences support grant which is awarded by the National Institutes of Health on a percentage basis*

The first provides ongoing support. The second provides money for special purposes such as carry-over funds in interim periods.

WHO SHOULD COLLABORATE?

At present, the decision to collaborate is usually made by one man who enlists the services of another man to assist him. The critical variable in making the decision to collaborate is probably mutual need. The need must be recognized by both parties. To broaden collaborative effort, then, it appears necessary to familiarize researchers in different disciplines with the potential contributions others could make to their areas of study. It also becomes important to identify the parameters of overlap, not only on a broad basis of problem area, but on a specific problem for a given researcher.

Cooperation among researchers can take place according to several different model relationships: that of project director and assistants; equal colleagues; sub-contract model. Each model listed varies according to locus of authority and decision-making. There are also built-in hazards unique to each.

Despite the fact that the decision to collaborate grows out of mutual need, and that such need exists in many cases, the impetus for undertaking segmental, focused collaborative study lies dormant. Aware of the problems and unable to see past their own vested interests, many members of the profession fail to collaborate when it would be beneficial. This could be identified as a widespread lack of understanding of the "big picture" in research.

It is difficult to get established investigators to collaborate because they are often already tied to a specific research method, or committed to a line of research. New PhD's might be more willing to undertake collaborative study.

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The problem is that the younger the researcher the less seasoned he is likely to be in his profession, and, therefore, even less equipped to collaborate than his older colleagues. Training programs would be necessary to orient the new PhD towards collaborative effort and prepare him for the problems involved. A second talent reserve for this method of research might be those interested in cross-cultural study.

Even though an individual investigator may not work on the same problem as his colleague in a cooperative effort, his decision to collaborate could provide him with a larger sampling of children, more expanded facilities, etc.

NURTURING COLLABORATIVE STUDY

Panel members agreed that an environment which fosters interaction between researchers must emanate from the profession. Three mechanisms might be utilized to assure that this occurs:

- *The profession should make it more respectable to work in collaboration by assuring appropriate professional advancement for such research.*
- *Funding priorities could be slanted toward interdisciplinary research.*
- *Better means for transportation of ideas should be employed.*

The obstacle of spatial organization might be overcome by concentrating collaborative study in research centers and institutes. This kind of organization would nurture a "research family" of multidisciplines. Decisions to collaborate could emerge from common experiences within such environments, and from discussions wherein mutual interests and problems are identified.

To stimulate interdisciplinary collaborative research panelists point out that it would be helpful to:

- *Encourage the beginning researcher to collaborate by training him in an interdisciplinary environment.*
- *Fund existing agencies, with the potential to become interdisciplinary centers, so that they can attract personnel to collaborate with researchers already there.*
- *Reflect the need for collaborative study through granting agency priorities and dollars.*
- *Fund construction and staffing of new interdisciplinary centers.*

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- *Establish multidisciplinary research teams in identifiable research organizations.*
- *Support individual study in related areas by incorporating this idea into interdisciplinary proposals, and encouraging sabaticals, leaves for study, and post-doctoral work in related fields.*
- *Sponsor interdisciplinary training seminars during summers for researchers from different fields in order to stimulate inter-area thinking.*
- *Make fringe area research visible through conferences, contract research, presentations and publications.*
- *Lay ground work for collaborative study through interdisciplinary training programs such as internships in related areas.*
- *Hold research conferences directed at specific areas which might benefit from collaborative study.*

There is no denying that nurturing collaborative study involves a kaleidoscope of problems. Panel members, acknowledging the problems, encourage colleagues in research to undertake more extensive collaboration and start the wheels in motion for a collaborative system in research.

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Manpower Needs in Research

In examining an expressed desire for improved communications and the expansion of collaborative studies, panel members looked at the *need for manpower* in the child development field. With statistics that already indicate a surplus of people in the field, economists have challenged the assertion that more people are needed in research. Society's need for research and people to carry it out, rather than society's current willingness to hire people, should be the criterion for judging whether or not there exists a manpower surplus or shortage.

A manpower study would be useful in determining evidence for such issues, although a single study would be only a short-term guide. Probably there should be means established for an ongoing look at numbers as well as types of jobs. In retrospect, for example, it would have been advantageous if someone five or ten years ago could have anticipated and predicted the current need for preschool teachers. The proposed National Planning Board could become involved in predictions and market research which would provide insight into problems of manpower in the child development field.

TYPES OF MANPOWER

Determining the number of researchers needed is incidental to deciding what *types* of manpower would supplement research. Panel members concluded that research needs:

- *synthesizers* or individuals who can correlate findings in major fields into new theories and into organized systems of information;
- *information specialists* skilled in communicating research findings to various publics;

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- *researchers with interdisciplinary training* to undertake collaborative studies;
- *program developers* who can blend both the basic and applied approach;
- *"second echelon" personnel* to fill positions which are natural extensions of the research organization.

The communications section in this report elaborated on the need for recruitment and training of *synthesizers* and *information specialists*. The shortage of interdisciplinary researchers who are competent in more than one field was previously listed as a barrier to R & D. A spokesman in the health profession re-emphasized this by pointing out that the problem of manpower in the health field cannot be solved by his profession alone, but by an amalgamation of professionals responding to public need for health care. A case has been made for the interdisciplinary researcher under "The Need to Collaborate."

In the area of program development there is almost no source of manpower; *program developers* must be specifically trained. From the training of developers, to the actual program development, to the application of the program is a lengthy process. The beginning of an MA+ program is emerging to facilitate the training of people with program development capabilities. Panelists noted that this type of training should be encouraged.

An example of *"second echelon" personnel* is the new category of child development consultant being supported by the National Institute of Mental Health (NIMH) through two year MA programs at George Peabody College in Tennessee, the Universities of Chicago, Michigan, California at Santa Barbara and Florida State. Consultants are research transmitters and research users in the allied health professions. Since these "technicians" fill roles which are essentially extensions of the research organization, there should be a second echelon in research where they could fit in comfortably. This second or intermediate level of people would be especially essential if research continues to move from the laboratory into the field.

NEW ENVIRONMENTS FOR TRAINING

How to train various new types of manpower requires a look at existing training programs. The university environment, for example, is presently unequipped to recruit, organize, and sustain a "second echelon" in research, because only professors and graduate students are physically there and supported by the academic system. In addition, university departments are not set up to provide for interdisciplinary research or synthesis of research findings. With each professor purposefully in a different field so that the department can represent a broad spectrum of interests, it is difficult to get people together to pursue common goals. There is usually not enough strength in any one discipline to work on a "program" of research. In regard, then, to the

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training of interdisciplinary types, synthesizers, and intermediate personnel, it appears that the university department may not be the place where this should be conducted.

What little effort is being put into training people in multidisciplines appears to be taking place according to two general plans: the use of an interdepartmental committee; the use of a common practicum experience by several different programs designed so that those in the practicum can communicate with each other.

Panelists agreed that a need exists to create environments which foster common practicum experience and greater interaction among researchers. Two specific environments suited for training the five types of personnel mentioned under manpower needs are *child study centers* and *child development institutes.*

Common practicum experiences can be had through the creation of *child study centers.* Such organizations would maintain a longitudinal study population where visiting scientists would be able to do research apart from that being conducted by the center. In addition to permanent staff, it seems that anyone who came in contact with this type of center would get multidisciplinary training. Examples of multidisciplinary child study centers are those at George Peabody College, the Universities of Washington and North Carolina. Even without physically establishing new centers, the environment provided by day care centers could encourage such interdisciplinary investigation. If money is going to be available in day care in the future, researchers could subcontract with centers to carry out research and training on their premises.

The child study center concept might flourish best free of university restrictions. On the other hand, there appears to be some problem in getting top researchers away from the campus setting. Educational Testing Service (ETS), the Regional Education Labs, and R & D centers provide examples of the upstream battle constantly taking place to recruit "good" people into a non-academic environment. It may be that child study centers should be created independently from the university but within a campus setting. Without a mandate from colleagues of increased respectability for the multidisciplinary researcher, problems in quality level recruitment will increase regardless of the setting.

Another alternative for training specific types of manpower is to do so within *institutes.* "Second echelon" personnel fit comfortably into the institute environment. Even without large budgets, institutes could provide a setting in which the interdisciplinary researcher could be trained, stay on to undertake collaborative study, and, generally, contribute to program research.

Institutes might function within a university environment. For example, child psychology could be pulled out of the general psychology department and strengthened into an institute. Institute people would teach university courses, but the arrangement would differ from a university department in two ways: staff members would have smaller teaching loads than the average department instructor; there would be a focused program of research and personnel hiring. A relatively small investment in stable positions has provided a great payoff in research productivity in the long run for the few

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institutes presently existing under such a set up.

A parellel in education would be to take educational psychology from a school of education and greatly strengthen it into an institute. In order to accomplish this, however, there would have to be motivation in terms of some kind of an endowment. A foundation or the federal government could encourage universities toward such an undertaking by saying, "if you will strengthen your educational psychology department and create an educational psychology institute, we will give you x number of dollars per year for x years." These newly created institutes could be thought of as arms of a new or proposed National Institute for Education. They would provide an environment where interdisciplinary studies could flourish.

FUNDS FOR TRAINING

Funding is a widespread problem affecting almost every area of child development research. Manpower training is no exception. For one thing, it is presently difficult to support new, broadly trained investigators because they are not receiving an adequate proportion of available funding. In order to reform training procedures and provide the quantity and variety of manpower needed in child development, it is necessary to consider changes in the present funding apparatus. Means of incorporating training with actual research should be explored so that research and training could exist on a continuum. Panelists suggest that:

- *Training funds should be given to students to work as apprentices rather than awarding scholarships with no attached responsibilities.*
- *Fellowships and assistantships should require a student to become a working research assistant after he has completed a stipulated number of student hours.*
- *Funding agencies should indicate support of multidisciplinary practicums in which students interact to discover how different programs correlate.*
- *Internship programs should be developed in which investigators work in a community, either with a master teacher or community official, in order to become competent in assessing and responding to public needs.*

Whatever the question regarding manpower, the tasks of recognizing talent and providing the best conditions for its development are urgent ones. As one member put it, "We wait not upon granting agencies, measuring instruments, etc., but upon discoveries **and** the ability to recognize them when they are made." Training is one of the most essential elements in accomplishing this.

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CONCLUSION

Changes are taking place in child development research, with or without coordinated effort for change. The time is right for researchers, funding agencies and their publics to rise to the occasion and channel future research efforts towards meaningful goals.

One purpose of this text is to stimulate further thinking about the need for overall planning in child development research. We have by no means exhausted the possibilities for desirable change, or the suggestions for implementing change.

In a more positive sense, it is hoped that the collective opinions herein concerning priorities, the need for synthesis, improved communications, expanded collaborative study, and new types of manpower will be catalytic in prompting further discussion and action on these issues.

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